(5)

AN ESSAY ON GLANDULAR SECRETION;

CONTAINING AN
EXPERIMENTAL ENQUIRY
INTO THE
FORMATION OF *PUS*:
AND A
CRITICAL EXAMINATION
Into an OPINION of
Mr. JOHN HUNTER's,
"*That the Blood is alive.*"

By JAMES HENDY, M.D.

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MDCCLXXV.

To the FACULTY of the MIDDLESEX HOSPITAL.

PERMIT me, gentlemen, to dedicate this ſhort treatiſe to your patronage. There is ſome propriety in my taking this liberty, as I wiſh this attempt to explain an intricate part of phyſiology, may be conſidered as the firſt fruits of a medical education, acquired principally, from the opportunities your kindneſs and friendſhip have given me.

I offer it as a ſmall tribute for the innumerable obligations I lay under to you; and I hope you will aceept it as a teſtimony of gratitude from,

Gentlemen,
your moſt reſpectful,
and very humble Servant,
London, January 1775.
JAMES HENDY.

Advertiſement.

THE Author humbly preſumes, that a preface to this publication is unneceſſary. He takes this opportunity, however, to beg the candid correction of the public where he has erred; and alſo to aſſure thoſe who do him the honour of peruſing this treatiſe, that, to meet their approbation is his chief ambition,

It is with great diffidence that he ventures to appear in public, and would by no means have done it, had he not been perſuaded to it, by the advice of ſeveral medical gentlemen, on whoſe judgment he lays much ſtreſs.

AN ESSAY ON GLANDULAR SECRETION.

Definition of Glands.

GLANDS are appendages to the ſanguiferous and lymphatic ſyſtems, and have the power of inducing changes on the fluids that are brought to them, or ſeparating particular parts from the general maſs.

The manner, by which theſe effects are produced, will be treated of in this eſſay, not indeed from a preſumption that I ſhould excel in illuſtrating this ſubject, but becauſe it appeared to be a part of phyſiology highly worthy of inveſtigation.

In proſecuting this ſubject, I ſhall divide it into three ſections: in the firſt, I ſhall treat of the general ſtructure of glands, and the propriety of the preceding definition, ſubjoining a ſhort view of the diviſion of glands; I ſhall then proceed to ſpeak more particularly of thoſe glands I term appendages to the ſanguiferous ſyſtem; and next, of thoſe that I ſuppoſe more properly to belong to the lymphatic ſyſtem. In the ſecond ſection, I ſhall give the moſt prevailing opinions, concerning the manner in which they produce alterations upon, or ſeparations from, the general maſs; and to each of theſe I ſhall point out the principal objections. In the third ſection, I ſhall offer what appears to me to be a *more* probable conjecture concerning the nature of ſecretion, and ſubjoin the reaſons that tend to ſupport this opinion.

SECT. I.

Concerning the Structure of Glands, &c.

CHAP. I.

Of the Structure of Glands in general.

THERE is no part of animal mechaniſm that has employed the attention of anatomiſts more, than the ſtructure of glands; and ſurely, from the importance of their functions to life and health, they juſtly merit a minute enquiry into their conſtruction. For, unleſs this be known, it muſt be impoſſible to acquire any correct idea of the mode by which they produce ſuch evident changes on our fluids. Indeed, every advance, without this foundation, muſt be vague and merely conjectural. I may remark, that this, as well as every endeavour of human induſtry to unfold the deep and hidden receſſes of wiſe Nature, have always required the moſt unwearied diligence, united with the beſt capacity. But genius and application are ſeparate gifts, and are ſeldom conjoined in the ſame individual ; yet there are not wanting inſtances, where theſe have united in the ſame perſon, and where it has ſo happened, it has given birth to very important diſcoveries. This is particularly exemplified in ſeveral profeſſors of the medical art. That the preſent inveſtigation was arduous, and required both theſe human powers united, we may, I think, eaſily perceive from the hiſtory of this part of anatomy; and we may alſo learn, from the diſputes of celebrated anatomiſts, that the minuteneſs and nicety of thi ſubject rendered obſervers liable to man fallacies. For the hiſtory of medicine i forms us, that at one period the medic world were ready to concur in opinion wi the ingenious Malpighi, and admit his ſu poſition that all glands were either veſicu or follicular. The arguments which this great man brought for his theory, and the ſupport it received from Dr. Boerhaave, gave it great plauſibility and weight ; in ſo much, that it is highly probable that we ſhould have adopted and retained this opinion, had it not been for the celebrated Ruyſch, who oppoſed Malpighi, and advanced an opinion, which though not totally new,* ſucceſsfully controverted his doctrine ; and this very accurate anatomiſt, by rendering the ſecrets of our organization evident and demonſtrable, and by oppoſing obvious objections to the concluſions of Malpighi, and alſo by bringing convincing proofs in ſupport of the opinion which he renewed, not only overturned the Malpighian doctrine, but at the ſame time eſtabliſhed his own, upon the beſt and moſt infallible foundation, viz. clear and deciſive experiments. For, by his very minute injections, he demonſtrated that

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* Edmundus King, Nehemiah Grew, and ſome others, who were prior to Ruyſch, had the ſame ideas of Glands with thoſe he advanced and ſupported.

that thoſe appearances which were taken for follicles, veſicles, or *cryptæ*, were, in fact, a congeries of veſſels. And, certainly, if this be the caſe, the arguments brought from the morbid ſtate, muſt be fallacious. But ſuppoſe, for an inſtant, we were to admit the follicular ſtructure of glands, and to allow proofs to be brought from the diſeaſed ſtate, we ſhall, even then, I am confident, find them very inadequate to the concluſion. Such arguments are to be found in Haller's *Elementa Phyſiologiæ*. In the firſt place, to invalidate this opinion, it is remarked, that theſe morbid veſicles are met with, where glands are not to be found, as in the cellular texture of different parts of the body. When they are found in glands, as indeed is very frequently the caſe, and if we ſuppoſe ſecretions are made in them, and obſtructions formed to their fluids making their exit from them by the excretory duct, ſhould we not expect to find the veſicles increaſed by the accumulation of the particular fluid which the gland ſecreted?

But this does not happen, as is evinced by hydatids of the liver, &c.

Should it be thought incumbent on the oppoſers of the Malpighian doctrine to prove from what ſources theſe morbid veſicles proceed, ſince they deny that they ſpring from the diſtenſion of veſicles, which they alſo reject as unconnected with the ſtructure of glands in their ſound ſtate; I can affirm, ſupported by the beſt authority, that they ſometimes happen in *loculi* of cellular texture, and that they often proceed from the diſtenſion of the ſpace between the valves of the lymphatic veſſels. The only part where hydatids do occur, and where lymphatic veſſels have not been demonſtrated, is the brain; and it is rendered highly probable, that they are not wanting in this important organ, by arguments brought by Dr. Monro and Mr. Hewſon.* Tyſon alſo advanced an opinion, that

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* Mr. Falconer, anatomiſt of London, informs me he has great reaſon to believe that he has injected lymphatic veſſels in the brain of the cod-fiſh.

they, in ſome inſtances, proceeded from worms, which opinion ſome men of judgment have admitted.

Though the Malpighian doctrine was queſtioned by ſeveral authors, yet Ruyſch, as he compleated its overthrow, has deſervedly acquired the principal honour. It is indeed to him that we are chiefly indebted; for if, by his art, he had not extricated us from thoſe difficulties which accompany ſuch a minute enquiry, we ſhould have perhaps been ſtill ignorant of this and many other parts of the *minutiæ* of anatomical ſtructure. This art has been ſo conſiderably improved by modern anatomiſts, and more eſpecially by the indefatigable Hewſon, that I am enabled to ſpeak with greater confidence on this ſubject.

CHAP. II.

Of the Variety of Glands, &c.

MOST writers, that I have conſulted, have defined glands, either from the external habit or the internal ſtructure; ſome from one, and ſome from the other: but it is evident, that a character taken from either of theſe, muſt be very imperfect. For if we only view the external figure of glands, the great diverſity which we find among them would lead us to exclude a great number, which we very properly term glands. The liver and teſticle are both glands, yet how different is their outward appearances? We ſhall find the ſame concluſion hold good, if we examine the internal conſtruction; for in this alſo there is a conſiderable diverſity. Some eſſential parts are wanting to certain glands, as for inſtance, the ſpleen, &c. are without excretory ducts, at leaſt what we call ſo, in the other glands. And indeed, where all the parts that enter the definition are preſent, *viz.* a congeries of minute arteries, veins, nerves, &c. they ramify in ſuch a variety of ways, as ſcarcely to appear to be compoſed of the ſame parts; as may be obſerved by comparing the pancreas with the kidney or liver. In my opinion, this great variety is what we ſhould naturally expect, when we conſider that each organ is to have its own particular effect; for, if ſize and conſtruction were the ſame, what elſe could be expected, but that they would all perform the ſame function? Beſides, there are inſtances where a change is produced on the coagulable lymph, merely by an organic pore, or more probably by an exhalant artery, as I ſhall hereafter endeavour to prove.

Baron Haller remarks the difficulty of forming a definition from any of theſe circumſtances, and includes their functions. After treating of theſe, he adds, " *Ex his omnibus colligitur, glandularum & organorum ſecretoriorum magnam varietatem eſſe, & parum*

rum omnino videri, quod diverſæ glandularum claſſes commune habent, cum neque fabrica conveniant, neque facie, neque functione; ut omnino ægrè definitionem invenias."* If a definition cannot be obtained from any of theſe, glands muſt remain undefined; but I humbly conceive, that an idea of a gland may be given by conſidering their general function. I have therefore advanced a new definition, not that I think it perfect, but becauſe it appears the leaſt liable to error, and to be the moſt unexceptionable; for it is certain that we call thoſe organs glands, which ſecrete. We extend this appellation indeed ſomewhat farther, *viz.* to thoſe which we ſuſpect to perform ſome ſuch office, as the ſpleen, &c. But until their uſe be publiſhed, we only call them ſo preſumptively, from their having ſomewhat of a ſimilar appearance to thoſe organs which we know to be glands. I have ſaid they are appendages to the ſanguiferous and lymphatic ſyſtems, concluding that the one ſyſtem does not merely ſerve to carry blood to every part of the body, or the other merely to abſorb from the ſeveral cavities, and return the whole of this fluid to the heart again, unacted upon; but that each of theſe ſyſtems has a power inherent in itſelf to act on the fluids which it carries. That this is the caſe, with reſpect to the arterial ſyſtem, is ſucceſsfully proved by the ingenious Hewſon, who obſerved different appearances in the coagulable lymph, when the arteries were acting differently. It has indeed been oppoſed to this concluſion, that very trifling circumſtances will cauſe remarkable diverſity in the appearance, from whence theſe concluſions were made. But it ſhould be remarked, at the ſame time, that all external circumſtances were the ſame; that the arteries themſelves only were found to be acting with different ſtrength at different times. The late moſt worthy profeſſor, at Edinburgh, Dr. Gregory,* in his courſe of practice, confirmed this, by aſſerting that he had ſeen different appearances in the blood, received in different veſſels, at the ſame bleeding, where the flow of the blood and every other apparent circumſtance was the ſame. Hewſon, by careful obſervation and experiment, found that the change in theſe caſes depends on the arterial ſyſtem's ſtate of action being changed. I conclude, therefore, with him, that the arterial ſyſtem is capable of changing the properties of a particular part of the blood, viz. the coagulable lymph. From hence, therefore, I think I am authorized to advance, that certain glands are added to this ſyſtem, to effect farther changes on the blood, or ſeparations from it.

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* "From all theſe circumſtances, we may gather, that there is a great variety of glands and ſecerning organs, and that the different claſſes of them have few circumſtances, in which they agree in common, as they differ much in ſtructure, appearance and function. Hence it is with the utmoſt difficulty, that one can pitch on a proper definition of glands."

I might perhaps with propriety have ſubdivided this part of my ſubject by ſaying, that glands are appendages to the arterial and to the venous ſyſtem; but I choſe rather to be general, than run the hazard of erring by endeavouring to be too minute. But yet, I think, this opinion might be ſupported to a certain degree, for the liver undoubtedly appears to be a venous viſcus. Indeed, it has been advanced, that the *venous* blood, which ſerves this organ, requires to be previouſly acted upon by the ſpleen. It is true, we obſerve, that the ſplenic vein, making the *vena portarum*, is, in a conſiderable degree, robbed of its coagulable lymph. Haller ſpeaks as follows, in his *Primæ lineæ*, § DCLXXXI, * "*Is ſanguis vix unquam coagulatus eſt.*" But that the ſpleen has an important office of its own, is a diſcovery lately made by that very excellent anatomiſt Mr. Hewſon. † Perhaps

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* Profeſſor of the practice of medicine.

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[*] "That blood is ſcarce ever coagulated."

[†] Some perſons, who were by no means maſters of Mr. Hewſon's reaſoning, have nevertheleſs ventured to criticiſe his opinion concerning the uſe of the ſpleen, &c. by which they not only ſhewed a want of judgment, in attempting to impugn a doctrine which they did not underſtand, but at the ſame time expoſed their ardent though fruitleſs endeavours to clip the wings of a *riſing genius*. He however could have no victorious oppoſer

haps the reaſon of the ſplenic vein's paſſing into the liver is merely becauſe it is the *neareſt and moſt convenient* way to return the blood to the heart. For, though the liver may require venous blood, yet it is improbable that it ſhould require another organ ſolely to prepare the blood for it ; as we obſerve that the male *ſemen*, which perhaps is the moſt elaborate, at leaſt the moſt important ſecretion, is formed in a gland independent of any other. In as far indeed, as every

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poſer to his towering greatneſs ; he could have no dangerous enemy to his future fame, but one, and that was death.

There have not been wanting perſons who have affirmed, that the uſe Hewſon attributed to the lymphatic ſyſtem was no real diſcovery ; and have placed it amongſt the ridiculous opinions of the ancients. They have laid much ſtreſs on the number of back-doors that Mr. Hewſon left, that he might eſcape the artillery of medical critics, and defend his hypotheſis.

Thus, ſay they, if it be advanced againſt Mr. Hewſon, that ſeveral animals have been deprived of their ſpleen, and ſtill that theſe particles have been completely formed, he immediately flies to the thymus gland. If it be then remarked, that after a certain age this gland is obliterated, he will retire to the lymphatic glands, and aſſure us that they are formed there. And laſtly, if it be oppoſed to his doctrine, that ſome animals have no every part is ſubſervient to the other, and altogether making one compleat whole, every part is dependent on the other, but no farther; and in this view I may ſay, that the kidneys prepare blood to be properly acted on by every gland in the body, by freeing it of its ſuperfluous water.—I ſhall ſpeak of the uſe of the ſpleen when I arrive at the lymphatic ſyſtem.

I might, from this diviſion of the ſubject, be led, by a diſtant analogy, to proceed ſtill farther,

no lymphatic glands, he then takes his laſt ſubterfuge and defends himſelf by retiring to the lymphatic veſſels themſelves.

To avoid this crafty oppoſition, for I cannot even term it ſpecious reaſoning, they ought to be informed, that it is the *lymphatic ſyſtem* which forms the red part of the blood, and that the ſpleen, thymus, and lymphatic glands are conſidered as parts of, or appendages to, this ſyſtem.

I cannot avoid remarking, that one gentleman who oppoſes Hewſon's doctrine, by the reaſoning I have juſt related, advances or rather ſupports an opinion which is overturned by the above arguments, without leaving himſelf a ſingle back-door to creep out at. He ſays, it is highly probable that the ſpleen is ſubſervient to the liver, and that it prepares the blood for that viſcus. He muſt permit me to aſk, how the blood is prepared for the liver, when the ſpleen is cut out?

farther, and I ſhall venture to do it, but with the greateſt diffidence. It is merely a ſuppoſition, viz. that certain glands, which continue to be the *opprobria* of the anatomiſt and the phyſiologiſt, may perhaps be conſidered as belonging to the nervous ſyſtem, and their uſes intimately connected with that ſyſtem. I mean the *glandula pinealis*, the *glandula pituitaria*, the ganglions of the nerves, and the renal *capſula*. That the *glandulæ pinealis* and *pituitaria*, and the nervous ganglions are ſo, we have ſome reaſon to ſuppoſe, from their ſituation and cloſe connection: that the renal *capſula* alſo makes a part of this ſyſtem, I am led to ſuppoſe from the peruſal of a paper in the *Acta Petropolitana*, written by DuVerney, in which he endeavours to ſhew, that there is a ſimilarity between this part and the ganglions of the nerves; but I muſt confeſs that the analogy does not ſtrike me.

This idea, I think, is better ſupported by a number of diſſections of a late accurate

obſerver,

obſerver, who found, on inſpection, that thoſe monſters, which are born with a diminution or total want of brain, have their renal *capſula* wanting in the ſame proportion.

According to the view I have now taken of this ſubject, the different glands of the human body may be arranged and enumerated in the following order:

Appendages to the ſanguiferous ſyſtem.

- 1ſt, *to the arteries*;
  - All the mucous glands, thoſe which moiſten or lubricate the ſeveral cavities, the ſalivary glands, *pancreas*, kidney, *mammæ mulierum*, teſticles, and perhaps the thyroid glands; indeed every gland, except thoſe mentioned under the other heads.
- 2dly, *to the venal ſyſtem*;
  - The liver.

Appendages to the lymphatic ſyſtem.

- The lymphatic glands, the *thymus* and the ſpleen.

And it is perhaps probable, that the following are parts belonging to the nervous ſyſtem:

The brain, the *glandulæ pinealis* and *pituitaria*, the ganglions of the nerves, and the renal *capſula*.

CHAP. III.

Further Conſiderations on thoſe Glands which are connected with the Sanguiferous Syſtem.

IT is probable that thoſe glands, which only ſeparate, or produce a ſmall change, are the moſt ſimple in their conſtruction. The kidney, however, ſeems an exception to this notion, for it appears to be as complex as any gland whatever. The only ſure method of judging when mere ſeparation happens, is to compare the fluid ſecreted, and obſerve if it is ſimilar in properties to any of the parts, which exiſt in the blood-veſſels. Where we find this is the caſe, we may juſtly conclude that it is a mere ſeparation. The tears, the fluid that is found in the ventricles of the brain, the matter of perſpiration and urine, are ſaline fluids, which I believe exiſted in the ſanguiferous ſyſtem. The matter, which is found in the ſeveral cavities of the body, I except from the number of ſeparated fluids. The *ſynovia*, which lubricates the joints, &c. is different in its general appearance from any part of the blood; it approaches nearer to a matter of an oily nature, which does not exiſt in the blood veſſels, unleſs it is carried thither, after being ſecreted and re-abſorbed from the adipoſe membrane: at leaſt, this is rendered highly probable, from the third chapter of the firſt part of Hewſon's Experimental Enquiry. That the interſticial fluids are not mere ſeparations, I am led to conclude, from the following obſervations. Though they coagulate, on being expoſed to the air, yet they differ greatly in the time required. The coagulable lymph of the blood completely jellies in ſeven or eight minutes, when expoſed to the air; but theſe fluids require thirty minutes for their coagulation. Beſides, there is another difference; for though expoſure to the air is proved to be the chief agent in coagulating the blood, yet long reſt will produce the ſame effect. Now, I will not aſſert, that the interſticial fluids are totally at reſt; yet they ſurely are by no means ſo violently agitated, as in the courſe of circulation. From hence it ſhould follow, that the delay in the cavities ſhould diſpoſe them to coagulate more ſpeedily when they are expoſed to the air, than the lymph taken from the veſſels while in their uſual motion. But this is found not to happen. The interſticial fluid differs from the coagulable lymph, more remarkably, in the time neceſſary for its coagulation, when neither is expoſed to the air, but in a ſtate of reſt, as is proved by Hewſon's experiments. [*See his lymphatic ſyſtem.*] For the coagulable lymph in the blood-veſſels was found to be coagulated at the end of ſix hours; whereas, the lymph in the lymphatic veſſels, which is the ſame with the interſticial fluid, was perfectly fluid after reſting twenty hours; and being then let out, jellied after being ſome time expoſed to the air.

I humbly preſume theſe differences are ſufficient to place this fluid among the altered ſecretions. I chuſe the ſtructure of this organ, which ſecretes the interſticial fluid, as an example of the moſt ſimple kind of gland,

And concerning the ſtructure of this organ three opinions have been advanced; viz. that it is an inorganic pore, admitting of tranſudation; that it is an organiſed pore, which is capable of ſeparating a particular part of the circulating fluid; and that it is an exhalant artery. The firſt of theſe opinions was advanced by Dr. Hunter, but is moſt completely overturned by Mr. Hewſon in his Experimental Enquiry. It therefore only now remains to determine whether it is an organic pore or exhalant artery, that forms the interſticial ſecretion. I am led to believe, it is an exhalant artery: for a mere organic orifice does not ſeem adequate to effect thoſe alterations, which have been obſerved to happen in an healthy ſtate of the ſyſtem; and it will appear leſs ſo, if we take into conſideration the morbid changes, and particularly the effect of the veſſels forming *pus*, of which I ſhall ſpeak hereafter.

Thoſe who have eſpouſed the idea of its being a mere organic pore, have argued, from ſuppoſing the interſticial fluid to be a mere water, ſaying that ſuch a pore was adequate to ſuch a function, and that the ſuppoſition of its being a veſſel would account for nothing farther. But finding that, in fact, a certain alteration is produced on the fluids, this reaſoning muſt be relinquiſhed. I ſuppoſe this exhalant artery to be ſomewhat ſimilar to thoſe veſſels which take their riſe from the red arteries, that are found to ſurround the fatty ſubſtance contained in the articulations of bones, &c.—which veſſels, it is very probable, ſecrete the *ſynovia* of the joints. I, the more readily, adopt this opinion of an exhalant artery, becauſe it agrees with Dr. Haller's, who, in treating of the termination of arteries, ſpeaks thus, § XLIII. "*Alius finis arteriæ eſt, quo in canalem exhalantem terminatur. Iſte finis ubique in corpore frequens eſt.*"*

Several inſtances of the more complex or compleat ſtructure of glands, might be brought; but it will be ſufficient for my purpoſe

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* "There is another termination of an artery, *viz.* in an exhalant canal. This ſort of termination is every where common in the human body."

to mention the liver, the kidneys, the teſticles, &c. I do not think it neceſſary to relate the ſtructure of theſe, becauſe a much more perfect knowledge than I could give in this ſhort treatiſe, may be acquired from many ſources. I ſhall however obſerve, that the appearance of *cryptæ* found in the kidney, was much in favour of the Malpighian doctrine; but Ruyſch affirms, that this appearance is owing to the wonderful manner in which the veſſels divide. Hewſon alſo proved, by his injections, that this was the caſe; and demonſtrated farther, that there is a ſerpentine veſſel running between theſe *corpora globoſa* and the excretory duct. Harderius, a cotemporary with Malpighi, in his *Exercitationes Anatomicæ*, appears to have a very clear idea of the compoſition of the *corpora globoſa* of the kidney. "*Exiſtimo tamen glandulas illas e quibus exterior renum ſubſtantia conflata eſt, aliud nihil præ ſe ferre quam vaſa variè gyrata et intorta, quæ poſt varios flexus et ambages tandem in papillas coeunt.*" * I do not, indeed, know a ſingle exception to the vaſcular ſtructure of glands. With reſpect to the termination of their excretory ducts, there is ſome diverſity which it may not be improper to notice. They either end on the ſurface of ſome cavity, in a bladder or reſervoir, or in both theſe ways. Of the firſt we have many inſtances, as in the *ſalivary* glands, the *pancreas*, &c.; of the ſecond, we have examples in the kidneys, and perhaps alſo in the *lacunæ* of mucous glands. We find a termination in both theſe ways in the liver. I thought it neceſſary to mention theſe circumſtances, becauſe it has been an opinion, with ſome, that the ſecretions were in ſome inſtances carried on in the reſervoirs: and it has been advanced that the *veſica fellea* formed bile. If theſe bladders were eſſentially neceſſary to ſecretions, no glands ſhould be without them; but I have obſerved that glands open into cavities, and are without this part. There is ſurely no more reaſon to ſuppoſe the gall-bladder forms bile, than that the urinary-bladder ſeparates urine, or that the *veſiculæ ſeminales* form *ſemen*, which we are certain is not the caſe. For, with reſpect to the laſt, if it was ſo, caſtrated males would ſtill continue prolific, which is not even ſuppoſed. I muſt therefore relinquiſh ſuch an opinion, and admit, with Dr. Haller, that theſe parts of certain glands are mere reſervoirs, for ſtoring up the ſecreted fluids, for the uſe or convenience of the œconomy; and that the principal change which they undergo, is to have the more fluid parts or ſuperfluous water, abſorbed, which is neceſſary for their ſecretion.

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* "I think however that thoſe glands, of which the exterior ſubſtance of the kidneys is compoſed, have no other appearance than that of veſſels differently contorted, which after various turnings and windings at length meet in *papillæ*."

CHAP. IV.

Of the Glands connected with the lymphatic Syſtem.

THAT there is a propriety in calling theſe glands, of which I am now to ſpeak, appendages to the lymphatic ſyſtem, I am well perſuaded; and though I cannot enter on a full detail of the proofs which may be brought in ſupport of this opinion, as ſuch an enquiry would far exceed the bounds of a pamphlet, and would be a diſſertation that would require more time and recollection than I can beſtow on it, I hope the particular experiments and judicious arguments which lead to this diſcovery will be publiſhed to the world among the poſthumous works of their invaluable author. It will be ſufficient to the preſent purpoſe, to mention a few leading facts, and the general concluſions. That this ſyſtem takes its riſe from the ſeveral cavities, and is by no means a continuation of the arteries, is ingeniouſly proved, by the celebrated anatomiſts Drs. Monro and Hunter, and Mr. Hewſon. Yet, it is a fact, that the red particles of the blood are found in this ſyſtem of veſſels.‡ This has been noticed by ſeveral obſervers, and, I believe, is not doubted by any. A queſtion naturally ariſes then, How ſhould theſe particles come thither, if not formed in this ſyſtem? It may perhaps be anſwered, that they are abſorbed. But ſurely this cannot be the caſe; for, in the firſt place, let us conſider and compare theſe particles with the ſcarcely viſible lymphatic orifices, which appear on the *villi* of the inteſtines, where it is to be preſumed they are as large as in any part of the body. We have no reaſon to ſuppoſe that capillary attraction ſhould cause theſe veſſels to take up particles at leaſt as large as their orifices. Beſides, before they could be abſorbed, they ſhould be poured out into the cavities of the body. But this is not the caſe; for they are not to be found mixed with the interſticial fluids, ſo far as they have been examined, although they might be imagined to happen, in conſequence of the ſtruggles of the animal while under the operation neceſſary for the experiment. But to ſhew that this is not in the leaſt degree probable, I muſt add, that theſe particles appeared in the *thoracic duct* immediately, both above and below a ligature, that was inſtantly applied; which would not be the caſe, if the particles were abſorbed. Is it not then with the higheſt probability, that I may anſwer the queſtion, and ſay, that theſe particles are formed in this ſyſtem, and in thoſe parts which I term its appendages, *viz.* the lymphatic glands, the *thymus* and the ſpleen, becauſe they are found to aid the lymphatic ſyſtem in its office, and make the ſyſtem compleat? I ſhall now proceed to ſpeak of the ſtructure of the lymphatic glands.

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‡ It is remarked by Dr. Haller, and brought as an argument to ſupport the old idea, *viz.* that the lymphatic veſſels are continued from the ſmall arteries.

1. The lymphatic glands are found pretty conſtantly in certain parts of the human body. When their arteries and veins are injected wit coloured liquor, theſe veſſels are found to divide ſo very minutely, that they appear t

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be compoſed of nothing elſe. And the ſame appearance is ſeen, if we inject the lymphatic veſſels with mercury. Hence I conclude that theſe two ſyſtems compoſe theſe glands principally; not that I mean to exclude, the nerves. Many anatomiſts have obſerved that they are replete with cellular texture, and are inveſted with a membranous tunic. The lymphatic veſſels, which enter theſe glands, very frequently divide, as is obſerved by Nuck and others, and unite again into one or more veſſels, at the part at which they make their exit. In ſome inſtances however, the gland is compoſed ſimply of a lymphatic veſſel convoluted, as is proved by unravelling them, and after this convolution they paſs on to the thoracic duct. Theſe glands are obſerved to be larger in young animals than in old ones.

2. The *thymus* is ſimilar in conſtruction to the lymphatic glands, except this circumſtance, that the lymphatic veſſels do not enter and paſs through, but take their riſe from this gland. It is alſo larger in young animals,

and gradually diſappears as the animal advances in years, and is often obliterated in the adult ſtate. I may remark alſo, that it is largeſt in ſome animals, not ſo much according to their preſent ſize, as in proportion to the ſpeed with which they grow. Thus it is larger in proportion in a calf, than in the human *fœtus*. I need not ſay any thing of the ſituation of this or the other glands, as I know not whether this would afford any aid to our reaſoning.

3. The ſpleen is remarked by moſt authors for the quantity of blood-veſſels that paſs into it, in proportion to its bulk. It is ſimilar to the laſt mentioned gland in giving riſe to numbers of lymphatic veſſels. It is ſaid alſo to be of a cellular texture.

Before I ſpeak of the uſe of theſe parts, I muſt refer to the Philoſophical Tranſactions, where proof will be met with, to eſtabliſh an opinion, which Mr. Hewſon advanced, *viz.* that the red particles of the blood are compoſed of two parts, a central or middle ſolid part, and a ſurrounding veſicle, or external covering. Mr. Hewſon, in the CIII. lecture of his anatomical courſe, made it appear extremely probable, that the lymphatic veſſels themſelves were capable of forming both theſe parts; but that, for the more compleatly performing this function, the lymphatic glands were found in the more perfect animals. That the veſſels themſelves are endowed with this power, is proved, by obſerving that ſome animals, that have no lymphatic glands, have this particle compleat.

In the infant ſtate there ſeems to be a greater demand, in the animal œconomy, for theſe particles; and, on this account, perhaps, young animals have an additional organ, which is obliterated as they advance in life: I mean the *thymus*. This gland ſupplies the central part, as appears by obſervation; for a number of theſe particles are brought from this organ by the lymphatic veſſels, which I ſaid, derive their origin from thence.

Though, from experiment, the ſpleen appears to be an important organ to ſanguification, yet ſeveral anatomiſts, and among theſe Mr. Hewſon, have cut out this viſcus, and the animal has continued to live; but whether with or without apparent diminution of this part of the blood, we have not had an opportunity of aſcertaining: we muſt leave this to future experiment. It is probable, however, that as there are other organs anſwering the ſame office, that they will, in ſome degree, compenſate for its loſs. For, not only in this, but in other parts of the œconomy, we perceive that nature has more methods than one of producing the ſame effect. The function of this viſcus ſeems to be, to add the flat veſicle to the central globule; for Mr. Hewſon obſerves, that the lymphatic veſſels, coming out from the ſpleen, are replete with theſe perfectly finiſhed particles: and what more proof can we have of the function of any viſcus? Do not we ſay that the liver forms bile, becauſe we perceive bile come from it? It appears farther, that this veſicular ſheath is formed from the coagulable lymph ; for we have obſerved above, that the blood contained in the ſplenic vein, ſcarcely coagulates ; and, that the coagulation depends on the preſence or abſence of this part of the blood, is too plain to be doubted.

SECT. II.

Concerning the Phyſiology of Glands.

CHAP. I.

The moſt remarkable Opinions concerning the Manner in which Glands produce their Effects on the general Maſs: with Objections to theſe Theories.

IN viewing the diverſity of hypotheſes, that have been framed, to explain the phænomena of ſecretion, I think it ſufficient to ſelect the moſt plauſible; and I ſhall firſt ſpeak of the opinion which Des Cartes ſupported, for it deſerves notice, both on account of its ſimplicity and ingenuity. This philoſopher ſuppoſed, that the ſecretory organs were ſimilar to ſieves, and that the ſeveral pores of different glands were adapted to the magnitude and figure of the particles which the organ was to ſeparate from the blood. However plauſible this theory may appear, at firſt ſight, the leaſt attention is ſufficient to refute it.

For, in the firſt place, it ſuppoſes that there are particles, of different ſize and ſhape, exiſting in the blood-veſſels; but of this there is not the leaſt proof: for, on viewing the blood with a microſcope, we only obſerve the red particles, and ſome of the central parts without their veſicular coat. Theſe red particles are all equal in ſhape and ſize, in the ſame animal, at the ſame period of life; but they differ in the ſame animal, at different periods of life, both in figure and magnitude, as may be noticed in the Philoſophical Tranſactions, before cited. Therefore, if theſe particles were ſeparated from the blood, they would require the ſecreting veſſels to be larger, and to have different forms at different times, which is not probable. The fact is, we do not obſerve theſe particles univerſally in the ſecreted fluids.

Secondly, it preſumes that all the ſecreted liquors ſhould previouſly exiſt in the blood-veſſels. If ſo, we ſhould find *bile*, *ſemen*, &c. circulating with the blood, which we do not. Beſides, if it depended on mere filtration, every ſmaller particle, and every more ſubtile fluid, would accompany the larger particles, or more viſcid fluids unacted upon: but, I believe, this is not found to be the caſe. Milk conſiſts of certain ſolid globules; but can we expect to find all the fluids with it unchanged? No; we find the caſeous matter of milk, which is ſuppoſed to be the moſt analogous to the coagulable lymph of the blood, to differ widely from it in certain properties.

Laſtly, according to this doctrine, the ſecreting veſſels of glands ought to be ſhaped differently; but the contrary of this is obſerved: for our veſſels, in a tranſverſe ſection, are found to be univerſally circular, excepting only the ſinuſes of the brain.

Another theory, equally ſimple with the former, ſuppoſes, that the particular fluids which the ſeveral glands ſecrete, exiſted there *ab origine* ; and that, as the blood paſſes through the blood-veſſels of the glands, each fluid attracts the parts which are ſimilar to itſelf, and repels the reſt. This hypotheſis, like the laſt, ſuppoſes that the ſecreted fluids were in the ſanguiferous ſyſtem *in propria forma*, which is diſputed, though Dr. Haller ſeems to think that they are. His words are theſe : "*Diximus in ſanguine omnes humorum claſſes reperiri, quas diverſa per cola de ſanguine oportet ſeparari.*"* That the elementary parts of all ſecreted liquors are contained in the blood, is abundantly probable ; but that they are acted upon and changed very conſiderably, is proved from the diſſimilarity of ſome of the ſecreted fluids from any part of the blood. But farther, this ſuppoſition is contradicted by fact; for, in an embrio, as Dr. Haller himſelf remarks, *lib.* VII. *ſect.* III. § XXXI. that the bile differs from what it is afterwards, both in colour and in taſte.

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* "We have ſaid, that all the claſſes of fluids are found in the blood, which muſt be ſeparated from it by different ſtrainers."

The third, and laſt opinion, I ſhall mention, is that which ſuppoſes glandular ſecretion to be performed by fermentation. In treating of this, I ſhall, firſt, endeavour to aſcertain what is meant by fermentation, according to the beſt definition of this operation.

Chemiſts do not ſeem to be agreed in their ideas of fermentation. It is, however, certainly neceſſary for me, in treating this ſubject, to diſcover what is meant by this operation; and, it appears to me, that the beſt and eaſieſt method of attaining this knowledge, is to examine the definitions generally given. I will firſt ſpeak of that which is given by a very excellent chemiſt, and which Dr. Pearſon has adopted in his Inaugural Diſſertation. "*Si qua in materie motus inteſtinus ſuboriatur, et per totam maſſam ſerpat, donec tota homogenea fiat, et ita mutetur ut quælibet pars ejus recenti materiei, ejuſdem generis ac illa ab initio fuerat, addita, motum inteſtinum in hac concitat, et univerſam ſibi omni ex parte ſimilem reddat; hi motus, et hæc ſub iis facta mutatio, fermentatio, et materies addita fermentationem ciens, fermentum dicuntur.*"*

In the firſt place, I muſt examine, if there is any *motus inteſtinus*; and, in order to this, I muſt find out, on what this phænomenon in fermentation depends. It is proved, by Dr. Black, in his Chemical Prælections, that it is the conſequence of the extrication of fixed air. Is it probable that any ſuch thing happens in our ſecretory organs? I ſhould preſume not. According to this definition, it is neceſſary, that the matter changed by fermentation, applied to freſh matter, ſhould convert it to its own likeneſs; that is, it ſhould act as a ferment. It is demonſtrable that this is not the caſe with our ſecretions. For, in caſes of *icterus*, the bile is evidently conveyed into the blood-veſſels, perhaps by abſorption; and every circumſtance favours fermentation equally, as heat, &c. yet, our fluids are not converted into bile. In caſes of *variolæ*, the *pus* is often abſorbed from the puſtules, and evacuated by the inteſtinal tube, and in this caſe, the *pus* is even ſuppoſed to have a particular ferment, though taken into the blood-veſſels; yet it does not act ſo as to convert our fluids into a ſimilar matter. Its action ſeems to to me, to be confined to the ſmall veſſels on the ſurface; for, if this particular matter had the power of acting as a ferment on our fluids, I cannot conceive why the diſeaſe ſhould ever ſtop, for freſh fluids are continually formed for it to act upon, and the variolous matter is preſent in a greater quantity, in the puſtules, at the end of the diſeaſe, than when it was firſt applied, in caſes of inoculation, to produce the diſeaſe. Theſe facts I cannot reconcile with the idea of a fermentation.——I ſhall prove hereafter, that *pus* is not capable of acting on any of our fluids as a ferment, and of converting them by that means into *pus*.

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* "If an inteſtine motion ariſe in any matter, and diffuſe itſelf through the whole maſs, until the whole become homogeneous, and be ſo changed that any part of it applied to recent matter of the ſame ſort, of which it had originally been, excite an inteſtine motion in it, and render the whole perfectly ſimilar to itſelf; theſe motions, and the change produced by their agency, are called *fermentation*, and the matter exciting the fermentation, *a ferment*."

Another very able chemiſt defines fermentation to be, "an operation, by which one compound is altered into a different compound, by a new mode of combination in the ſame elements." It would be very difficult to prove that ſomething of this ſort does not take place in our fluids, though, at the ſame time, no proofs can poſſibly be brought to ſhew that it does happen. But then this is too vague a definition to be admitted, for it does not agree properly with the principal and beſt known ſpecies of fermentation, *viz.* the vinous; in which it is certain, that the ſame elements do not conſtitute the new compound, as is well known, from the copious ſeparation of fixed air, and depoſition of lees and other feculent matter, which happens during this fermentation.

Beſides, if the other definition was correct, or, in other words, agreeable to the phænomena of fermentation; this cannot be ſo, as may appear by comparing them. For though, in the former, a decompoſition and different arrangement may probably take place; yet, from certain matters, we always have the ſame reſult. Thus, the farinaceous matter of vegetables, is converted into ſugar, ſugar into wine, and ſo on, till at laſt it putrifies, and theſe appear to me, to be only different ſtages of the ſame operation, which invariably happen. But the contrary of this takes place in ſecretions, and, there is by no means this ſteadineſs; for, though they are all formed from the blood, yet, they are wonderfully varied in their qualities.—It may, perhaps, be ſaid, that animals have the power of converting vegetable matter, by the proceſs of digeſtion, and other animal proceſſes, into an animal nature, without admitting theſe ſeveral ſtages, which indeed, does not appear to be the caſe. But then, it muſt be remarked, that animal matters, in their progreſs to decay, paſs into the putrefactive fermentation. The living principle of animals has a power, however, of reſiſting this diſpoſition to decay, in a conſiderable degree; though many circumſtances in life are highly favourable to this operation, as the degree of heat, &c. Putrefaction being the next ſtep which animal matter is diſpoſed to paſs into, it would undoubtedly be expected, that every change by any proceſs, in the leaſt ſimilar to fermentation, ſhould verge towards putrefaction, and the degree of putrefaction ſhould be in proportion to the change. Our fluids therefore, that are moſt altered, ſhould, under equal circumſtances, run faſter into putrefaction,

faction, or, in other words, ſhould ſooner putrefy than thoſe which are leſs changed. The following experiments determine the fact to be otherwiſe.

It has been pretty generally believed, that *pus* in particular, was formed by fermentation, till ſome gentlemen, whom I ſhall mention hereafter, controverted it. However formed, it is manifeſtly, a change produced on our fluids by the ſuppuratory inflammation, and therefore, proper for the experiment.

EXPERIMENT I.

I took three vials of equal ſize, No. 1. a little more than half-filled with freſh ſerum; No 2. filled to the ſame height with ſerum, mixed with red particles; No. 3. filled in the ſame manner, with bland *pus*, which was given me by Mr. Hewſon, and which had the ſame day been taken from the cavity of the *pleura*. Theſe I placed, unſtopped, all under the ſame circumſtances,

cumſtances, in my window, and examined them from time to time, and obſerved they putrified in the following order. No. 2, was fætid firſt, and after it No 1, emitted a fætid ſmell, while No. 3, was without any, and continued ſo for ſome time after. This would not have happened, if the *pus* had been formed by a proceſs, ſimilar to the putrefactive fermentation.

As this, and indeed all the experiments, were made merely for my private ſatisfaction, without the leaſt idea that they were to appear in print, I was not perhaps, ſufficiently exact in noting down the particulars, but paid the greateſt attention to the general concluſions they lead to. As, however, I had an opportunity of repeating this experiment, and obſerving the particular time that each began to putrify, with greater nicety, I determined to do ſo.

EXPERIMENT II.

Edinburgh, June 21, 1774, I obtained about two ounces of good *pus*, taken an abſceſs, which, unfortunately however, was mixed with ſome of the red particles. I alſo got the ſame quantity of freſh ſerum, ſeparated from red particles, and an equal quantity of ſerum, loaded with red particles. The blood, from whence I took theſe parts, was taken from a patient, labouring under an inflammatory diſeaſe, two or three hours later than the abſceſs was opened, and the *pus* obtained. The three vials containing theſe, I marked A, *pus*; B, ſerum; C, ſerum with red particles. Theſe I placed in a baſon with Farenheit's thermometer, and expoſed them to the ſun. The degree of heat, for the moſt part, was in the morning, about 58; between twelve and one, from 66 to 70; and in the evening, 58. I alſo, at times, when the ſun did not ſhine on them, endeavoured to promote the putrefaction, by placing the baſon in a water-bath, in a degree of heat as near 98 as poſſible, but it ſcarcely ever reached ſo high.

June 26. In the evening C ſhewed a ſlight degree of fætor, which was increaſed during the night, ſo as to be very evident the next morning. A, and B, quite ſweet. June 27th, ditto.

June 28th. C, fætid; A, and B, ſweet. 29th, C, fætid, A, and B, as yeſterday, 30th, as yeſterday. July 1ſt. in the morning, A, emitted a diſagreeable ſmell, B, ſtill ſweet; C, more fætid. July 2d. A, was evidently fætid; B, gave an unpleaſant ſmell; C, more fætid. July 3d. A, more fætid; B, evidently putrid; C, very fætid.

Though, in this experiment the reſult is different, yet, when the cauſe of this difference is aſcertained and explained, we ſhall find it will in no wiſe contradict the concluſion drawn from our former experiment.—For, we muſt conſider, that, as the addition of the red particles cauſed C to putrify ſuch a length of time before the pure ſerum B; ſo alſo the red particles, that were unavoidably mixed with the *pus* in opening the abſceſs, muſt alſo promote the putrefaction of A; and it leaves us not the leaſt reaſon to doubt, that, if it were not

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for the mixture of the red particles, which forwarded the putrefaction of the *pus*, that it would have remained much longer ſweet than the ſerum; at any rate, it proves, that *pus*, even when containing the red particles, is not ſo putreſcent as the ſerum with red particles. So this experiment alſo is ſatisfactory.

CHAP. II.

An Endeavour to determine whether Pus *is formed by a Fermentation; and whether it can poſſibly act as a Ferment, either on the Solids or Fluids.*

THAT *pus*, does not in the leaſt depend on fermentation, and alſo, that it is incapable of acting as a ferment on the effuſed fluids, I am convinced, from the experiments, I ſhall now relate, and which were made while I was ſtudying at the Middleſex Hoſpital, at London.

EXPERIMENT III.

I took a thin ſlice of mutton, and placed it in a deep ulcer of the leg, which was in a granulating ſtate, and produced laudable *pus*, leaving ſome of the *pus* to act as a ferment. I covered the ulcer with a piece of ſmooth lead, and bandaged it up. I viewed it ſometime after, and found that the piece of mutton was every way leſſened, but it was not converted into *pus*; on the other hand, it was very fætid, differing exceedingly from the ſecretion that the ulcer formed at that time.—From this experiment, it appears, that *pus* is not produced by a fermentation of the ſolids; and alſo, that the cauſe of loſt ſubſtance, in caſes of abceſſes, is not owing to the ſubſtance being converted into *pus*, and therefore that it muſt depend on ſome other cauſe. For though the piece of fleſh, in this experiment, was ſo acted upon by the *pus*, or more probably by putrefaction, I cannot conclude that this effect would happen in an equal degree on a living part. For, on the contrary, we know that there is a power in life, of reſiſting the action of any cauſe that tends to its deſtruction, proved by applying cauſticks of equal ſtrength and ſize, one to the living body, another to the dead: we ſhall find the ef-

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ſect on the dead body much more conſiderable than on the living. Mr. Hewſon, in the 7th chapter of his Lymphatic Syſtem, hath advanced, that the waſte of ſolid parts in abſceſſes, may depend on the preſſure of the fluid contained in the cyſt, which preſſure will increaſe, in proportion to the accumulation of the matter within, till at laſt it burſts where there is the leaſt reſiſtance. I may obſerve, in confirmation of this, that preſſure ſeems to have this effect in a very conſiderable degree, as we may perceive by what happens in aneuriſms, where there is preſſure againſt a bone; for it deſtroys the bone, as I have ſeen, particularly in a caſe of aneuriſm of the curvature of the *aorta*, which deſtroyed a large portion of the *ſternum.*—This fact, I believe, cannot be accounted for from any idea of fermentation; but preſſure, in both inſtances, appears to be the cauſe, in whatever manner it produces the effect.

With a view to aſcertain whether the effuſed fluids were fermented into *pus*, I made the following experiment.

Experiment IV.

Into the ſame ulcer, which continued to form good *pus*, I poured a quantity of freſh ſerum, which was ſeparated from the blood of a patient labouring under an acute rheumatiſm, and exhibited an inflammatory buff. I left, as in the laſt experiment, a ſmall quantity of *pus*, to act as a ferment, and covered it with lead. When I examined it, I found it was become very fætid. At firſt I made this experiment with the ſerum of blood which was not inflammatory; but Mr. Hewſon ſuggeſted to me, that perhaps, in order to the formation of *pus* by fermentation, it required that the ſerum ſhould be acted upon by inflamed veſſels, and deſired me to repeat the ſame experiment, with blood which ſhewed an inflammatory cruſt. The reſult of both, however, according to expectation, was the ſame.

In order to determine whether it was the coagulable lymph that was changed into to *pus*, the following experiment was made.

EXPERIMENT V.

In the ſame ulcer, with all circumſtances as recited above, I placed a ſlice of the buffy coat of inflamed blood, *i. e.* the coagulable lymph coagulated. As in the other caſes, ſo in this, it became fætid. This experiment, however, is not concluſive; for it may be ſaid, that the lymph is poured out in its fluid ſtate, and is acted upon before it coagulates. The following therefore was made, which is more deciſive.

EXPERIMENT VI.

From the blood of a patient affected with an acute rheumatiſm, I collected the lymph by little and little, taking advantage of the ſlowneſs with which the blood coagulates, and placed it in the ulcer, covering it up from the action of the air. The quantity of lymph was ſmall; but, as in the other experiments, ſo in this, it gave a degree of *fœtor*.

From theſe experiments I am led to conclude, that *pus* is not the conſequence of fermentation; and if a morbid alteration does not depend on this chemical proceſs, I think there is leſs reaſon to ſuppoſe that the natural ſecretions are produced by any ſuch means. I ſhall hereafter ſay, that *pus* is probably formed by the exhalant arteries, at leaſt in ſome inſtances. The only fermentation which uncontrovertibly takes place in the animated ſyſtem, is the putrefactive, and this can only happen, in any conſiderable degree, where the *vis vitæ* is feeble, and at any rate cannot proceed far, conſiſtent with life. Perhaps thoſe diſeaſes, which are ſaid to be produced by contagious ferment, may probably depend on the ſpecific *ſtimulus* of each matter, cauſing morbid alteration in the action of the moving fibre or nervous ſyſtem, and by this means alone affecting our fluids, and by no means deriving their origin from fermentation. In both ideas there are difficulties, but to me it is more conceivable, and more conſiſtent with animal life, that various alterations or deviations from the proper action of the veſſels ſhould happen, than to ſuppoſe that there are ſo many different fermentations; eſpecially, when I reflect on the great diverſity there is in the *ſuccus proprius* of vegetables, and conſider that the moſt mild and the moſt acrid receive nouriſhment from the ſame water, as is experimentally proved by Du Hamel, &c. The honourable Mr. Boyle, in his work concerning the uſefulneſs of experimental natural philoſophy, *part* II. *eſſay* ii. ſays, "that rain water, which in its paſſage through a vine or an apricot-tree, or the like plants, is turned into a ſweet liquor; in its paſſage through thoſe plants that bear lemons and barberries, is tranſmuted into a liquor ſharp enough to corrode not only pearls, but coral, *lapides cancrorum*, and other hard concretes, as *ſpiritus vitrioli* would do." Dr. Hope alſo obſerves, that the mallow and the arum growing in the ſame garden-pot, and ſupplied with the ſame water, &c. retain their proper and diſtinct natures; for while the one is mucilaginous, the other is acrid. I am led to aſk, therefore, whether it is probable, or even poſſible, that this variety of changes on water ſhould be effected by fermentation? I ſhould ſuppoſe not; for, as every circumſtance is the ſame, except the difference of vegetable organization, I cannot conceive that the changes can depend on any thing but the different conſtructions of the different vegetables; which from hence derive the power, they poſſeſs, of changing the water they imbibe.

CHAP. III.

An Enquiry whether the Blood is alive.

IT might be expected, that, whilſt I argue againſt fermentations taking place in our ſecretions, I ſhould take advantage of an ingenious ſuppoſition, which I lately met with, in the Medical Commentaries, I mean an idea of Mr. Hunter's, viz. "that the blood is really alive:" for it certainly would be of uſe to me, in the courſe of this treatiſe, if I could admit the fact. But I avoid founding the leaſt upon ſo hypothetical an idea; and, indeed, as in my next general diviſion I mean to conſider the blood as altogether paſſive, and merely acted upon by the living ſolid, I think myſelf obliged to endeavour to refute this opinion, before I can poſſibly be at liberty to proceed to reaſon as though the blood be really not alive.

With this intent, I will examine Mr. Hunter's proofs of this opinion ſeparately, and in order.

Firſt, "Becauſe it unites living parts, in ſome circumſtances, as certainly as the yet recent juices of the branch of one tree unite it with that of another." If the juices of vegetables were proved to be alive, ſuch analogous reaſoning might be admitted; but no one ever ſuppoſed it. If we attend to the operation of grafting, we ſhall not conclude with Mr. Hunter that it is the recent juices that is the cauſe of the union in trees; for the gardener does not depend on the juice, but is chiefly careful to oppoſe certain parts of the graft to ſimilar parts of the ſtock. He applies theſe together, as ſoon as the neceſſary inciſions are made; not that he requires the yet recent juices to unite the parts, but becauſe the vegetating powers are ſtill vigorous: and on this account he prefers a particular ſeaſon of the year.

Secondly, "Were either of theſe fluids to be conſidered as extraneous or dead matters, they would act as *ſtimuli*, and no union would take place either in the vegetable or animal kingdom." This argument, Mr. Hunter imagines, is ſtill farther eſtabliſhed by the following experiment, *viz.* the teſticle of a living cock, being introduced into the belly of a living hen, upon injecting the liver of the hen, was injected along with the liver, as it had come in contract with it and adhered to it. As Mr. Hunter ſuppoſes that the fluids, or any matter whatever, if dead or extraneous, ſhould act as *ſtimuli*, *i. e.* according to his idea, ſhould produce a particular proceſs, which muſt terminate in throwing off the dead or extraneous part from the living or ſound parts; I would aſk, why the teſticle of the cock did not cauſe this proceſs to begin? I avoid engaging in the diſpute, whether a part of an animal when ſevered from a living animal, may or may not be ſaid to have life.

In ſome of the leſs perfect animals, it is a fact, that the ſeparated part is capable of becoming a complete animal; alſo that the muſcular parts of the perfect animals retain their irritability ſome time after reſpiration has ceaſed.

We therefore will not venture to affirm, that the teſticle of the cock ought to be conſidered as a dead part, but ſurely it muſt be allowed, that it was very extraneous with reſpect to the liver of the hen.

But Mr. Hunter obſerves, "that this union of living parts, takes place in certain circumſtances." I can only find that it happens in caſes of inflammation, and the phænomena of inflammations are ſufficient to explain this union, without the ſuppoſition of the fluids being alive. For we obſerve, that inflammation diſpoſes the inflamed veſſels to elongate, and the union of parts depends on this elongation; as we may obſerve in adheſions of the inflamed *pleura*. Du

Hamel, relates an inſtance, where the ſpurs of a cock placed upon the comb, when it was inflamed, fixed there, and grew. From theſe and all the other proofs, which Mr. Hunter himſelf has brought, there is much more reaſon, I think, for ſuppoſing that the union of parts depended rather on the life and elongation of the veſſels, than to ſuppoſe that the fluids are alive which theſe veſſels carry.

Thirdly, "The blood becomes vaſcular, like other living parts, and the *coagula*, as Mr. Hunter affirms, in the extremities of arteries, after amputations, may be injected by injecting theſe arteries."

This indeed may be the caſe, and nevertheleſs, the fluids which theſe veſſels carry, may not be alive. For I have juſt obſerved, that inflammation cauſes the veſſels to elongate, and after amputation the ſtump is inflamed; hence veſſels may ariſe and paſs through this *coagulum* and anaſtomoze; but they by no means take their origin from the *coagulum*. For if this were poſſible, I ſhould expect that thoſe clots, that are termed falſe conceptions, ſhould be vaſcular, which is by no means the caſe; and that the blood ſhould become vaſcular in every inſtance, where it is extravaſated. Beſides, the blood, in its coagulated ſtate, cannot poſſibly be ſaid to poſſeſs life, according to Mr. Hunter's own idea of life. In order to prove this, I muſt employ his arguments againſt himſelf.

In the firſt place, Mr. Hunter, in conſidering the blood as alive, views it as a fluid; as is evidenced by his having alledged, "that in the nature of things, there is not a more intimate connection between life and a ſolid, than between life and a fluid." By coagulation, the blood loſes its fluidity and becomes ſolid. Mr. Hunter ſeems to admit that the coagulation of the blood is a morbid change, produced by the ſtimulus of expoſure which happens, he ſays, as certainly, as the cavity of the *thorax*

thorax or *abdomen* inflame, from the ſame cauſe. I alledge, admitting for an inſtant, that the blood whilſt fluid was really alive, that it muſt, according to Mr. Hunter's own words, be conſidered, when in its coagulated ſtate, as being really and certainly dead. If this be the caſe, it undoubtedly is a very unfavourable circumſtance, to be brought as an illuſtration of the blood poſſeſſing life. But what is life, in the ſenſe in which Mr. Hunter adopts that term? It may be gathered, from obſerving that he conſiders a muſcle, cut out of the body, to be alive as long as it continues capable of being acted upon by *ſtimuli* of any kind; that is, as long as a part retains any irritability. Mr. Hunter agrees to this, by ſaying that the muſcles of a turtle continue alive a great while after the animal, as one whole, is dead. Query, does the *coagulum* poſſeſs the leaſt degree of this kind of irritability, or life, as Mr. Hunter is pleaſed to term it? No, it certainly has loſt all irritability; and therefore Mr. Hunter himſelf, not only agrees that coagulation is a morbid change, but by his own arguments, proves that the *coagulum* of blood is really a dead matter.

Fourthly, "Blood taken from the arm, in the moſt intenſe cold which the human body can bear, raiſes the thermometer to the ſame height, as blood taken in the moſt ſultry heat." This Mr. Hunter conſiders, as a ſtrong proof of the blood's being alive, as living bodies alone have the power of reſiſting great degrees both of heat and cold, and of maintaining, in almoſt every ſituation while in health, that temperature which we diſtinguiſh by the name of animal heat. This is indeed, a proof that life can reſiſt the action of external cold; but is by no means a proof, that this power exiſted in the blood. On the contrary, blood drawn in ſeveral different circumſtances of the air, in leſs than two hours will be cooled to the degree of the ſurrounding atmoſphere, as I am convinced from often repeated experiments. I therefore aver, that the blood, independent

dependent of the animal ſyſtem, cannot retain heat, which indeed, ſeems to be connected with life. Now as, in fact, we find it cannot retain its heat, it cannot be alive.

Fifthly, "Blood is capable of being acted upon by a *ſtimulus*; for it coagulates on expoſure, as certainly as the cavity of the *thorax* or *abdomen* inflames from the ſame cauſe." Any perſon would be laughed at, if he were to affirm that a jelly of any kind were alive, becauſe in a certain heat it continues fluid, and on diminution of this heat, or on expoſure to cold and air, it coagulates.

Sixthly, "The more it is alive, *i. e.* the more the animal is in health, the ſooner it coagulates on expoſure; and the more it has loſt of its living principle, as in the caſe of violent inflammation, the leſs it is ſenſible to the *ſtimulus* produced from its being expoſed, and the later it coagulates." It ſhould ſeem, from this, that it is a mere paſſive kind of life that the blood enjoys; for it is totally dependent on the animal powers, which cauſe it to circulate. I muſt alſo remark, that it muſt be a very curious kind of life which it poſſeſſes; as the more it is alive, the eaſier it is affected by a *ſtimulus*: now, it is the reverſe in every other kind of life; for the weaker the principle of life is, the more violently it is affected by ſlighter *ſtimuli*.

But perhaps Mr. Hunter will advance, that the blood *which really is alive*, when acted upon by inflamed veſſels, and violently agitated by this means, falls then into a comatous ſtate, ſo that it is not ſo much affected by *ſtimuli*. I might alſo add, that the blood is found coagulated, where the *ſtimulus* of expoſure is not applied, as is proved by effuſions in the brain. The coagulation here, takes place merely from reſt, which Mr. Hunter, I preſume, will maintain to be a *ſtimulus*.

Seventhly, "The blood preſerves life in different parts of the body. When the nerves going to a part are tied or cut, the part becomes paralytic, and loſes all power of motion, but it does not mortify. If the artery be cut, the part dies, and mortification enſues." What keeps it alive in the firſt caſe? Mr. Hunter believes, "that it is the living principle alone which can keep it alive; and he thinks that this phænomenon is inexplicable on any other ſuppoſition, than that it is ſupported by the blood." I believe no one ever doubted that blood was eſſentially neceſſary to the life of a part. If this experiment proves any thing, it is that nouriſhment is chiefly conveyed by the arteries, and not by the nerves; but even in this view it is hardly ſatisfactory. For though the larger branches of nerves were tied or cut, yet the almoſt infinite diviſion of nerves would lead us to conclude, that a number of theſe ſmall branches were ſtill left, at leaſt ſufficient to continue the circulation of the fluids, which depends on the action of the ſolids produced by nervous influence.

And though a part deprived of blood does mortify, yet the fluid, which ſuſtains it, may not be alive itſelf. At leaſt this ſeems by no means neceſſary, for the whole animal body cannot exiſt long, independent of certain *ingeſta*, taken into the ſtomach. But becauſe this is highly neceſſary to preſerve life, we cannot from thence infer that the *ingeſta* muſt have life.

To conclude this chapter, I affirm, that the blood, conſidered as a fluid, and in this light merely Mr. Hunter views it, cannot, in the nature of things, have life: for life evidently conſiſts in the performing certain functions of an active kind; and for the performance of theſe, a certain organization is abſolutely neceſſary. Fluids do not admit of organization, therefore they cannot be alive.

SECT. III.

A more probable Conjecture concerning Glandular Secretion.

CHAP. I.

The Arguments which ſupport this Conjecture.

ALthough the opinions which I have mentioned, concerning glandular ſecretion, have, upon being attentively examined, proved altogether unſatisfactory, yet I do not think this intricate part of the phyſiology, totally incapable of being explained. I propoſe, in the next place, to offer ſome attempt towards an explication of it.

With this intent, I muſt premiſe that all ſecretions are produced in conſequence of life, and that, independent of it, they cannot proceed. From hence we may juſtly infer, that ſecretions are very intimately connected with the living principle or nervous ſyſtem.

The animal machine is wonderfully conſtructed of ſolids and fluids: the former poſleſs the living or active principle, the latter are paſſive, and totally dependent on the ſolids, which are endowed with the power of deriving nouriſhment from the fluids, for their increaſe, and the other purpoſes of the machine.

The vaſcular ſyſtems, which contain the fluids, can only be affected by nerves, in as far as they change the ſtate of motion. This, I believe, cannot be doubted of by any. If then our ſecretions depend totally on the influence of the nervous ſyſtem, and this ſyſtem gives the power of action to the veſſels; it appears, that it muſt be the effect of this action that determines the nature of our ſecretions. And in the preſumption that ſecretions depend on the influence of the nervous ſyſtem, I am ſupported by ſome of the greateſt authorities in medicine, viz. Hoffman, &c.

I derive proofs that ſecretions depend on the action of the veſſels, in the firſt place, from conſidering the ſecretions in their ſound ſtate, and the alteration produced on them, without the leaſt morbid change in their conſtruction. For this purpoſe, I may obſerve how inſtantaneouſly the milk in the *mammæ* is changed by ſudden fright; and alſo that the liver, as may be learned from the very ingenious Dr. Maclurg, has its ſecretion increaſed by anger; and that, in general, the ſecretion of bile has a conſiderable relation to the paſſions. I would here aſk, what poſſible effect the paſſions can have on the fluids which are carried by the veſſels, except by their influence in changing the ſtate of action of the veſſels, which contain the fluids? Now, that emotions of the mind, and paſſions, have a great power on the actions of veſſels, is a univerſal obſervation, and demonſtrated by the effects of modeſty or ſhame, in producing bluſhing, and by fear, producing paleneſs. It is from hence that the paſſions are properly diſtinguiſhed into ſedative and ſtimulant. The glandular parts on which the ſedative paſſions exert their influence have the action of their veſſels diminiſhed, and in conſequence of this effect, the quantity of ſecreted fluid of ſuch parts is alſo leſſened. The contrary of this takes place from the ſtimulant paſſions.

Sedatives and ſtimulants applied to the body, where glands are ſituated, produce effects on the ſecreted fluids, by diminiſhing or increaſing the action of the ſecerning veſſels; thus cold leſſens the quantity of perſpiration, while heat increaſes it.

The variety of motions which the nerves are capable of cauſing, in the larger arteries, are almoſt infinite, as we learn from thoſe writers, who treat of the pulſe. And that an equal variety may take place in the action of the ſmall veſſels we have not the leaſt reaſon to doubt. Indeed, I have proof of it in the phænomena of topical inflammations; for we feel pulſations, when a part is inflamed, which were not felt before.

As it is fully aſcertained, that the nerves give origin to animal motion, is it not probable that theſe motions are variouſly modified, according to the ſtructure of the organ, on which the nerves exert their influence? Thus, if that coat of the veſſels, which has a muſcular power, is weaker in certain parts than in others, ſuch parts muſt of courſe have a weaker action. And alſo, if the elaſtic coat, which appears to aſſiſt in the action of the veſſels, by counteracting the contraction of the muſcular power differs in ſtrength in different parts, (for it is probable the alteration of diſtenſion depending on the elaſtic coat,* and the contraction depending on the muſcular coat that conſtitutes the action of the veſſels in every part of the body does the ſame) variety of actions in the veſſels will undoubtedly ariſe from this ſource. Hence, therefore, it may juſtly be preſumed, that a variety of motions ſhould be produced, depending on the relative ſtrength of theſe coats: and it is undoubtedly probable that the coats of the ſecretory veſſels do differ, and moſt likely in thoſe circumſtances I have mentioned. Dr. Haller ſeems to be ſomewhat of this opinion, and on his judgment I always lay the greateſt ſtreſs. His words § CCXXI. are as follow: "*Denſitates arteriarum minimarum & denſitates pariter oſculorum ſecretoriorum diverſas eſſe poſſe, nihil repugnat & ſuadet in majoribus ramis certa obſervatio.*"* And

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* If the veſſels are ever diſtended beyond their elaſticity, by an over quantity of blood, as is the caſe, perhaps

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perhaps ſometimes, in plethoric habits, then the elaſtic becomes an aſſiſtant power to the muſcular coat, by endeavouring to reſume its proper diameter.

* Nothing hinders the denſities of the ſmalleſt arteries, and the denſities of the ſecerning orifices too, from being different; and certain obſervation perſuades us that the caſe is ſo in the greater branches.

in the ramifications of the veſſels I have noticed an extenſive variety, which I have often ſeen in moſt beautiful microſcopical preparations, made by Mr. Hewſon. Dr. Haller has alſo obſerved, this great variety, § CCXIX. "*Aſpergillum in liene referunt arteriolæ rubræ, confertim ex trunculis oriundæ; pencillum in inteſtinis, ſerpentis in renibus, ſtellam in hepate, cincinnum in teſte, circulum in uvea efficiunt. Non fruſtra vero fabricæ hanc diverſitatem a Creatore factam eſſe, non obſcura conjectura eſt.*"*

This diverſity of ramification, no doubt, has its effect, in determining the nature of our ſecretions; but it is chiefly to the action of the veſſels in the different glands that I attribute the greateſt power of changing our fluids. Dr. Cullen ‡ inclines to this opinion, as appears from § CCLXXXI. of his Phyſiology ; ſpeaking of ſecretions, he ſays, "till we can diſcover theſe more clearly, we may, in the mean time, obſerve, that the action of the veſſels of the ſecretory organ, has a conſiderable ſhare in determining both the quantity and quality of the ſecreted fluid.

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* The minuteſt arteries reſemble a watering-pan, in ie ſpleen, ariſing in crouds from ſmall trunks; a bruſh the inteſtines; ſerpents in the kidneys; a ſtar in the ver; a curl in the teſticle; a circle in the *uvea*. Now is perfectly obvious, that this diverſity of ſtructures as not given in vain by the Creator.

Indeed the capacity of the ſecerning orifices of glands merely, and the velocity of motion, which the blood acquires by the action of the large arteries, ſeem to have conſiderable influence in determining the quantity, and perhaps, in ſome inſtances, the quality of thoſe fluids, which are conſidered as ſeparations from the general maſs. To illuſtrate this, it is obſerved, that the matter of perſpiration is increaſed by exerciſe, and that the kidney in a ſpaſmodic ſtate of its veſſels, ſeparates a limpid fluid, without the admixture of the mucilage, which is ſuppoſed to be generated by a ſmall degree of putrefaction taking place in the blood. Theſe circumſtances, however, aſſiſt us very little in accounting for the phænomena of ſecretions; for, in general, they are independent of the velocity of the blood in the courſe of circulation.

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‡ Profeſſor of the practice of Medicine at Edinburgh

CHAP. II.

The Subject continued.

FROM the reaſoning in the preceding chapters, I humbly preſume, that the ſecerning veſſels of glands have a plaſtic power on the fluids, and by this power produce their effect on them.

I am confirmed in this opinion, by reflecting on the power of the large veſſels, in producing changes according to their mode of action, and by conſidering the different ſtate of ſome of the interſticial fluids, which vary in properties, according to the healthy ſtate of the animal. What may be uſed as a further argument for this opinion, is, that the different appearances of *pus*, depend totally on the different degrees of inflammation, or, in other words, on the different action of inflamed veſſels.

Mr. Gaber is the author of an ingenious paper in the *Miſcellanea Taurinenſia*, in which he endeavours to prove, that *pus* may be formed by the putrefaction of ſerum. In repeating this gentleman's experiments, I find the phænomena, in as far as I attended to them, as he has related: but yet, I can by no means agree with him, that the depoſition of putrid ſerum is *pus*: his ingenuity has indeed carried the analogy a great length; yet, I humbly think, that the ſediment of ſerum differs from *pus* in ſeveral eſſential circumſtances. A conſiderable time is required for this depoſition, whereas *pus* is completely formed in a few hours, a ſhort time only being neceſſary for the fluid parts to be abſorbed; and it is from this part, being abſorbed, that it acquires its opake and viſcid appearance.

It is true indeed, that * "*Pus laudabile fere ſemper fætet, ſed parum, et vix ſenſibiliter,*"

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* Laudable *pus* almoſt conſtantly has a diſagreeable ſmell, though in a ſmall degree, and ſometimes hardly perceptible.

ter:" yet, it muſt have happened, to every obſerver on this ſubject, to have been frequently found perfectly ſweet, (particularly in the large cavities) and having very much the appearance of good cream. Serum does not depoſit its ſediment until there is a very perceptible *fætor*, that is, not until putrefaction is advanced a conſiderable way; I cannot therefore ſuppoſe *pus* to be produced by putrefaction, until it can be proved, that it is always fetid; which cannot de done.

I know very well that *pus* will become putrid, as well as every animal fluid; but I ha ve already proved, that it is not ſo putreſcent as ſerum.

It may indeed be advanced, in favour of *pus* being formed by this proceſs, that, in ſome inſtances, where it remains but a ſhort time, it is found to be very fetid; and an example may be brought from the very fetid ſmell, which a ſtump gives, in ſeveral of the firſt dreſſings after amputation. I think, however, that *pus* acquires this ſmell, in conſequence of the red particles oozing out from the ſmall divided arteries, and promoting putrefaction. This idea is proved by noticing, that, after a certain time, when theſe veſſels may be ſuppoſed to be cloſed, the *fætor* goes off.

As I have taken pains, therefore, to aſcertain that the formation of *pus* does not, in the ſmalleſt degree, depend on a fermentation of the ſolids or fluids, this is the proper place to mention on what I think it does depend. And here, I exactly coincide in opinion with thoſe who have affirmed that *pus* is ſecreted by inflamed veſſels. The firſt perſon who advanced this very ingenious doctrine, was, I believe, Dr. Thomas Simpſon, profeſſor of medicine and anatomy, at St. Andrew's. For this gentleman, in a public oration, *anno* 1726, *de re medica*, ſays, " *In medicorum diſceptationibus, nihil eſt celebrius, quam ut tumores, fluores, morboſque omnes,*

quiquidem evacuationibus omnino cedunt, ad humores ſtatim pravos & malignos referant; nempe quia neſcii ſint, quomodo liquores aliam atque aliam induere poſſint formam, niſi vel fermentum aliquod, vel alia quæpiam nova inſolenſque materies interveniat. Verum, ut de hac re clarius certiuſque conſtituatur, ex familiariſſimis maximeque pervagatis corporis humani caſibus; poſtulo, ut mecum velint vulneris, quod muſculoſæ alicui corporis benè ſani parti infligitur, ordinem & progreſſum pauliſper expendere.—Primo, igitur ex hiantium vaſorum oſculis pleno rivo profluit ſanguis ſincerus; deinde, iiſdem vi ſua contrahente paulatim conniventibus, liquor tenuior & pallidior, donec tandem omnino coeant, cum levi partis ſauciæ inflammatione; quam ſequitur mox ſanguinis confluentis remora, & ex ea paſſim febricula, donec vaſa dudum contenta denuo ſe ſponte ſua remittant, ſinantque in communi vulneris alveo deponi liquida, ubi ſtagnantia & a calore corporis modicè ſeta, in unam albidam ſuique ſimilem materiem, quam vulgus pus *appellat, concreſcant. Quod ſi jam piſum, vel*

vel ejuſmodi quid in plagæ fauces injiciatur, ſimulque arceatur aeris contagium, efficitur ut pus illud quamdiu voles fluat; prorſus ut hoc pacto repente exiſtat emergatque nova quædam glandula. Contra, ſi vulnus quoquo modo irritatur, nimiumve comprimitur, liquida ex integro mutabuntur, aliamque ſpeciem ferent, id quod chirurgi optimè norunt.——Hinc ſequitur facilius eſſe nihil, quam ut corporis ſecretiones humoreſque, ſine ulla novæ materiæ vel fermenti acceſſione, mutata ſolum vaſorum ſecernentium diametro numerove, immutentur."*

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* " Nothing is more common, in the writings of phyſicians, than to aſcribe, without heſitation, tumors, diſcharges, and all diſeaſes which yield, in any manner, to evacuations, to vitiated and corrupted fluids; for which no other reaſon can be aſſigned, than that they do not underſtand how fluids can aſſume different forms, unleſs through the intervention of ſome ferment, or ſome other new and unuſual matter. But that this point may be determined, with greater clearneſs and certainty, from the moſt familiar and common caſes that occur in the human body, let them attentively conſider, for a little, the order and progreſs of the appearances of a wound, which is inflicted on any muſcular part of a ſound body.—Now, firſt of all, there iſſues, in a full ſtream, from the mouths of the yawning veſſels, pure blood; then, the

Since this Dr. Morgan, has laboured the ſame ſubject at greater length, and with the greateſt ſucceſs, as appears from his excellent inaugural diſſertation *De Puopoieſi* anno 1763, to which I refer the reader for ſatisfactory arguments.

An additional proof, that *pus* is formed by a ſecretion depending on the action of

the veſſels contracting by little and little by their own contractive power, a thinner and paler liquor; until, at length, they entirely ſhut, while a ſlight inflammation takes place, in the wounded part, which is ſoon ſucceeded by a ceſſation of the efflux of the blood determined to the part, and that by a ſlight fever, until the veſſels, formerly ſtretched, ſpontaneouſly relax themſelves, and depoſit their liquid contents in the common cavity of the wound; where ſtagnating, and moderately cheriſhed by the heat of the body, they are converted into a whitiſh and perfectly uniform matter, commonly called *pus*. But now, if a pea, or any thing of that ſort, is introduced into the mouth of the wound, the conſequence is, that the *pus* diſcharges as long as you pleaſe; ſo that, in this manner, a new kind of gland, as it were, ſuddenly ſtarts into exiſtence; on the contrary, if the wound is in any way irritated, or too much compreſſed, the fluids will be entirely changed, and aſſume another appearance, as ſurgeons well know.——Hence it follows, that there is nothing eaſier than to change the ſecretions and fluids of the body, without any addition of new matter, or a ferment, by only altering the diameter of the ſecerning veſſels."

the veſſels, may be derived from the chirurgical treatment of inflamed parts. Experience has convinced ſurgeons, that a certain condition of *pus*, is connected to a certain ſtate of the part inflamed; and accordingly, to abate or encreaſe the inflammation, they apply, either ſedative or ſtimulant applications; whereas, if the fluid was fermented into *pus*, after its extravaſation, we ſhould not expect that they would proceed in ſuch manner; but, that inſtead of applying thoſe applications, which exert their influence, in altering the ſtate of action of the inflamed veſſels, they would be chiefly ſolicitous to employ ſuch methods as might promote the fermentation.

Mr. Hewſon has adopted this opinion, (ſee p. 117, 118, 119, of his Lymphatic Syſtem) and has farther advanced, that in ſome inſtances, it is ſecreted by the exhalant arteries, C. 9, where *pus* is found in the cavities without the leaſt exulceration. The doctrine of fermentation being previouſly exploded, this fact amounts almoſt to a demonſtration, that *pus* is a ſecretion, depending totally on the ſuppuratory inflammation, and alſo that in ſuch inſtances it muſt be formed by the exhalant arteries.

Finding then, that the exhalant arteries, under different circumſtances, produce different effects, as well as do the larger arteries by their alteration in this mode of action, all that remains, in order to eſtabliſh that ſecretions, in general, are performed by the action of the ſecerning veſſels, is to transfer this reaſoning to thoſe glands, which I treated of as more complex or compleat glands. This I have in part done, where I affirmed that they were univerſally vaſcular; for though, at the ſame time, I remarked that there was a difference in the ramifications of the veſſels in different glands, &c. yet this, and other varieties, which no doubt exiſt in their conſtruction, are only ſuch as are proper for them, as they are to effect a greater change on the general maſs. But that there is probably a ſimilarity between the exhalant veſſels, and the ſecerning veſſels of glands, appears to be the opinion of the illuſtrious Haller, by his aſking § XLIII. "*Annon adfinis eſt exhalanti fabricæ omnis ſecretio in glandulas veras ſive cryptas?*"

Now, as the nature of the interſticial fluids, as I have endeavoured to prove, depends on this ſimple organization of an exhalant veſſel; ſo perhaps, the ſpecific qualities of all the ſecretions are formed by thoſe arteries, which, in their continuation, conſtitute the beginning of the excretory ducts of glands.

I humbly preſume, that this opinion is ſomewhat ſupported by remarking, that ſecretions are not neceſſarily affected by changes in the general circulation; becauſe the excretory duct is removed at a greater diſtance, and the velocity of the blood is previouſly much diminiſhed, by the various contorſions of the minute arteries. And perhaps, this opinion may be confirmed by the obſervation, that the ſecretions are increaſed by ſtimulating their excretory ducts, that is, by increaſing their action.

The lymphatic veſſels of the lymphatic glands, with thoſe of the ſpleen and *thymus*, according to this view, are to be conſidered as adequate to the excretory ducts of other glands; for theſe veſſels appear to anſwer a ſimilar uſe.

FINIS.